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sonian Institution upon the development of libraries, the organization and work of societies and the publication of scientific literature in the United States,' by John Shaw Billings; 'Relation between the Smithsonian Institution and the Library of Congress,' by Ainsworth Rand Spofford. The mere enumeration of these descriptive titles explains the scope of the articles, and shows how fully the editor, Dr. G. Brown Goode, covered the entire field of the work within the Institution and its contact with-out during the first half century of its existence. In an appendix William Jones Rhees chronicles in order the principal events in the history of the Smithsonian. A full index closes the volume.

Twenty-four engravings and process-pictures of superior excellence are scattered through the book; they embrace views of the Smithsonian Institution and of the Hodgkins medal, with portraits of Smithson and of many of the Regents. As respects the typography, press-work, paper and binding no pains have been spared to make the book worthy of its subject. A small number of copies were bound in white vellum. For bibliographers the exact title is appended: *The Smithsonian Institution, 1846-1896. The History of its First Half Century*, Edited by George Brown Goode. City of Washington, 1897. Pp. x+856. Royal 8vo. Illustrated.

H. CARRINGTON BOLTON.

*THE DIGNITY OF ANALYTICAL WORK.**

It will doubtless be conceded by all that in the choice of the field to which one proposes to devote his life-work a number of things should be consulted. Among these may be mentioned not only mental capacity and the opportunities for training by courses of study which may be available to

him, but also what may be termed natural inclination or love for the work. Just how much work should be given to each of these elements is a query not easily answered, but few will deny that genuine interest in or real love for the field of work chosen should be allowed as great sway as possible. Those of us who have gotten far enough along in our life-work to be able to look back somewhat, and to see and to differentiate the causes that have shaped our line of effort, know full well that circumstances beyond our control, rather than our inclinations and desires, have in many cases determined our course, but the fact nevertheless remains that for the best results, for the attainment of even moderate success, one's efforts must be in an agreeable field and his heart must be in his work. Fortunate is the man for whom circumstances so shape themselves that he is able to pass his years in the field of his choice and spend and be spent in work that is congenial to him.

Assuming now that, for most of us, circumstances and conditions have been such that we are spending our lives in the field of our choice, let us consider, for a moment, a tendency that seems to be a concomitant of those thus fortunately situated. Do we not occasionally find in ourselves a disposition to magnify the importance of the field in which we happen to be engaged? Are we not somewhat inclined, quite naturally perhaps, to think that our field of work is more important than that in which others are occupied? Does not the theoretical chemist, whose inclinations lead him to spend his time in writing reactions and building structural formulæ of wondrous architecture, often feel within himself that his work is on a higher and nobler plane than that of the patient analyst who has furnished the data which he uses? Does not the organic chemist who delights in the study of the carbon compounds, who can

* Presidential address delivered at the Washington meeting of the American Chemical Society, December 29, 1897.

repeat for you series after series of chemical bodies, differing from one another by the constant addition of an element, or group of elements, in whose vocabulary 'types,' 'substitution,' 'replacements,' 'condensations' and 'isomers' are familiar words, and who when a new organic compound is discovered cannot rest until he has found to what series and what place in the series it belongs, or what its relations are to other bodies in that marvelous structure, based on the element carbon, which the studies of the last half century have reared before our eyes—I say does not this organic chemist oftentimes feel that he is engaged in a field far more worthy of study, to which is due much more consideration, than to that of his inorganic brother who devotes days, and perhaps weeks, to unraveling the constitution of some obstinate silicate whose crystalline form gives little help, and whose oxygen ratio is hidden or obscure? Or, again, does not the physical chemist oftentimes think that, with the tools of his more especial field, with his specific heats, his vapor densities, his heat of chemical combination and his ions, he is quite competent to solve all problems worth solving in the realm of chemistry, and that those who are engaged in other lines are far below his standard and can be looked down upon with quite pitying sympathy? Still once more, do we not often see the pure chemist, whose battle cry is 'original work for the work's own sake,' claim for himself the highest seat in the synagogue, and refuse to join his efforts with those of others whom he regards as his humbler brethren, viz., those working in the field of applied chemistry, in securing the benefits of organization to extend and widen the borders of our science? Finally, not to make distinctions, do we not frequently see the analyst, who knows so well how necessary it is to have the trained and skillful hand and the acute and watchful brain both working together and at the

same time, in order to secure the accuracy without which his work is worthless, claiming for his field that it is the foundation upon which our science rests, and that those who spend their time in locating the position of an atom in its molecule, or in finding the relations of an organic compound to other members of its series, or perchance in inventing long names for new compounds in which all the resources of the ancient Greek and Latin are brought to bear, to reveal in one word the constitution of the compound—I say does not the analytical chemist often regard these workers as unworthy to be called chemists?

Now, far be it from me to say that this partiality of each for his own field is blameworthy. We can, indeed, conceive of cases in which this partiality may be carried a little too far, but within proper limits not only is it not blameworthy, but even, as it seems to us, it may be praiseworthy for one to magnify the importance of the work in which he is engaged. A just and proper estimate of the value of his own work, a reasonable pride in his chosen science, or in that paddock of his science which it has fallen to the share of each to care for and cultivate, and indeed a moderate, though necessarily a somewhat partial, comparison of himself and his field of labor with others, even though that comparison is somewhat to the detriment of the others, are not always necessarily bad. On the other hand, such pride and such comparisons tend to stimulate to renewed activity, tend to sustain in the perplexities and discouragements of work, and tend to keep one's effort concentrated on the work which he can do best. Looked at in this light, the generous rivalry of one branch of our science with another, or the pardonable pride of each in his own chosen field, and even in his own work, may be a distinct advantage, and I know you will bear with me a few minutes, while I, with proper modesty and in the

true spirit, I hope, try to magnify a little the field of analytical work.

To my mind, then, it is just and proper to take pride in analytical chemistry, because of the power which a properly conceived and executed analysis has of explaining difficulties. A few illustrations will, perhaps, make this point clear, and I am sure I shall be pardoned for giving illustrations from my own experience, rather than historical ones.

Some years ago, after a passenger coach on the Pennsylvania Railroad had been through the hands of the car cleaners, it was noticed by some of the officers that the paint on the outside looked very badly, and had apparently been injured by the cleaning. A careful examination by the paint experts revealed the fact that the varnish was nearly all gone, and in some places the paint itself partially removed. As a matter of discipline, the car cleaners were called to account, and requested to explain why the paint and varnish had been so badly injured. Their reply was that with the soap that was furnished for car cleaning no better results could be obtained. This statement was, of course, received with a grain of allowance, it being well known to railroad operating officers that almost universally when anything goes wrong, and the men are called to an account, the materials are blamed. However, in order to give the men the benefit of the doubt, a sample of the soap was obtained and submitted to analysis, when it was found that this soap actually contained over three per cent. of free caustic soda and about seven per cent. of sodium carbonate. It is evident that this soap had been very carelessly made from cheap materials, and, since it is well known that water solutions of both caustic and carbonated alkalies are fairly good solvents for dried linseed oil and other constituents of paint and varnish, it is clear that the defense of the men, in this case at

least, was legitimate and that the soap was really at fault. It may be added, for information, that the circumstances above described led to the preparation of a specification for common soap, in which the amount of free and carbonated alkali was limited to very low figures, and that no similar difficulty of destruction of paint and varnish has since occurred.

Another illustration from a different field will emphasize the power of an analysis to explain difficulties. A lot of boiler-plate was at one time received at the Altoona shops from one of the best makers. In this lot of forty or fifty sheets two were found which gave difficulty in flanging, this operation consisting, as is well known, in bending over the edges of the sheets while hot, nearly at right angles to the balance of the sheet, in order to enable it to be joined to other sheets in the boiler. The two sheets referred to cracked in the bend, although the remainder of the lot gave no difficulty from this cause. The workmen being thoroughly experienced, and the practices of the shop being excellent, the cause of the failure in the case of these two sheets was not apparent. An analysis of samples from each of these sheets, however, showed 0.35 per cent. and 0.36 per cent. of carbon respectively, while analyses of samples from other sheets in the same lot showed in no case above 0.12 to 0.15 per cent. of carbon. The explanation of the difficulty seemed now quite clear. The shops had been supplied for a long time with the softer grade of steel, and the methods and practices in use were those applicable to that kind of steel. No wonder, then, that with the harder grade difficulty should arise, as actually happened, and but for the analysis this might have passed into shop traditions as one of those unexplained and unexplainable crotchets of steel which both the makers and practical users of this metal delight in constantly bringing forward.

A single illustration further will, perhaps, suffice on this head. A few years ago a shipment of some three hundred freight axles was received at two different shops on the Pennsylvania Railroad, from an entirely reputable maker. Some of these axles were used for repairs, and some went under new cars. Scarcely had they gotten into service, however, before difficulty began to arise. The axles began to break. Indeed, one of them broke before the car had been turned out of the shop yard, one broke into three pieces before the car had made 150 miles, and in less than three months eight had broken. Each of the broken axles was sent to the laboratory, and a careful study of the case made, with the hope of discovering the cause of the failure. An examination of the freshly fractured ends of several of the broken axles showed that for a little distance in from the circumference the fractured steel presented an appearance quite different from that given by the remainder of the fracture. Moreover, a line of demarcation between these two apparently different kinds of steel in the same axle could be clearly traced. Accordingly, it was decided to make analysis of borings from near the circumference and near the center, and see whether this would reveal anything. It may be stated that the axles were known to have been made from Bessemer steel, and should normally have contained not more than 0.10 per cent. of phosphorus. The analysis of the borings from near the circumference of the axles in no case gave figures up to this limit, while the borings from the center of the axles in no case showed less than 0.16 per cent. phosphorus, and in some cases the amount was as high as 0.24 per cent. Those who are familiar with the methods in daily use in modern steel works will, from these figures, at once understand the cause of breakage of these axles. For the benefit of those who are not, it may be well to explain that

in most modern steel works large ingots are now the rule, and that in large ingots, which take considerable time to solidify from the molten condition, analyses show that some of the constituents of the steel are not uniformly disseminated throughout the mass. This separation of the constituents during cooling, technically known as 'segregation,' is characteristic of the carbon, the phosphorus and the sulphur. Furthermore, the segregation appears to be worst in the upper third of the ingot, so much so that many specifications now require the upper third of the ingot to be removed, and not used at all in making the articles the specifications call for. This much being stated, it is clear why our axles broke. They were made from badly segregated steel, perhaps from the rejected upper thirds of a lot of ingots, the balance of which were used for other purposes. Subsequent correspondence with the parties furnishing the axles gave good grounds for belief that such was the case. For the comfort of those who ride on railroads, it may be added that the 300 axles were at once withdrawn from service, and that since that time a chemical and physical specification for both passenger and freight axles has been prepared which is believed to preclude the possibility of such axles as are described above being received by the Pennsylvania Railroad.

These illustrations of the power of an analysis to explain difficulties could be prolonged to almost any extent, but I spare you. Furthermore, I should not like to be understood as claiming that every puzzle, every difficulty or every state of affairs in nature where the reasons for the phenomena which we find are not apparent at sight can be explained by a chemical analysis. Our knowledge is far too limited for this. Moreover, many cases could be cited in which an analysis throws no light whatever on the situation; but, notwithstanding this, an experience of some twenty

years in seeking out the causes of things, as a necessary preliminary to the intelligent modification of practices and methods in connection with a great corporation, has continually impressed me more and more with the very great help which a properly conceived and executed analysis can give in cases of difficulty.

But, again, I take pride in the field of analytical work, because of the opportunity which thoughtful analytical work affords for finding new things. The careful, thoughtful, observant analyst is constantly on the verge of either being able to add to his own knowledge or of being able to contribute something to the general progress of our science. And here, again, I must be pardoned for using as illustrations cases which have arisen in the laboratory of the Pennsylvania Railroad Company.

A few years ago, in our laboratory, we began to get ready to make our analyses of the samples of steel which were designed ultimately to be the international standards for the analysis of iron and steel. Before starting in on these samples, however, it was deemed prudent to do a little preliminary work on some other samples, with the idea in mind of seeing whether apparatus and method were satisfactory. Accordingly, four separate and distinct determinations on the same sample were made for carbon, using the double chloride of copper and ammonium to release the carbon, and burning in oxygen gas. The four determinations agreed with each other within 0.01 or 0.02 of a per cent., and were regarded as fairly satisfactory. But as the work was important, and as some parts of the apparatus had not worked quite satisfactorily, it was decided to repeat the four determinations. Meanwhile a new stock bottle of solution of the double chloride had been made exactly in the manner that had been our custom for some time previous. When the second four determinations were ob-

tained they differed from the first by more than a tenth of a per cent. I need not weary you with the details of our hunt for the cause of this discrepancy, how every point in the apparatus was tested one after another, how various modifications were tried, how combustions were made on crystallized sugar to check ourselves, and how finally we located the difficulty in the double chloride of copper and ammonium solution. These details have all been published.* Suffice it to say that, as the result of this work, together with subsequent work by other chemists, it is, we believe, now generally accepted that the commercial ammonium double salt contains carbon in some form, probably pyridine, that its use as a solvent to release the carbon from iron and steel is unreliable, and that the substitution of the potassium for the ammonium double salt overcomes these difficulties. The point which I especially want to emphasize is that, in trying to do a little careful analytical work, we struck a new and apparently hitherto unsuspected source of error in one of the oldest and best established methods of iron and steel analysis.

Another illustration will, perhaps, make this point still more clear. In the regular course of work, at one time a silicon determination was made in a piece of tire steel which had been sent by an officer of another railroad for information. The figures found by our analysis were 0.14 per cent., these figures being sent to the officer above referred to. A little later word was received that an analysis of a sample from the same tire by another chemist gave 0.28 per cent. as the content of silicon. This, of course, led us to look over our work, with the idea of finding where the cause of the discrepancy lay. A careful examination of our weights and figures showed that it was not an error of calculation. Accordingly, we decided to duplicate our work, need I

* *Trans. A. I. M. E.*, 19, 614.

say, with the expectation of finding that the other chemist had made a mistake? Judge of our surprise when we found that our second analysis confirmed his figures exactly. Our first and second analyses had been made by the same method, and by the same operator, working on borings from the same bottle, and the cause of the discrepancy between the two was not, therefore, at first sight apparent. On carefully questioning the operator, however, as to exactly what he did at each step of the method, a clew was obtained, which, when followed out, cleared up the whole difficulty and ultimately led to a modification of the method. The silicon in these samples was determined by what is known as Drown's method, which consists in dissolving the steel in nitric acid, adding sulphuric, heating until white fumes of the latter acid appear, to render the silica insoluble, dilution with water, filtration, washing and weighing. The difference between our two analyses consisted simply in this, that in the first case, after the dilution with water, there being considerable work in hand, the vessel was allowed to stand overnight before filtration, while in the second case filtration immediately followed dilution. Subsequent work on this point showed that in this method silica is not completely dehydrated by heating in concentrated sulphuric acid in presence of iron salts, but is apparently rendered colloidal and sufficiently dehydrated, so that if filtration follows soon after dilution fairly accurate results will be obtained. On standing after dilution, however, this colloidal, undehydrated silica, apparently goes into solution again. Indeed, we were able to get on this same sample, anywhere from one-eighth up to the full amount of silicon present, by varying the time of standing after dilution, the longest time covered by our experiments being about four days.

Perhaps I may venture to give you still

one more illustration of how, in the course of analytical work, new and apparently hitherto unnoticed reactions may be hit upon and modifications of methods result. Every chemist who has done much work in determining phosphorus in iron or steel, by the reduction of the molybdic acid of the yellow ammonium phosphomolybdate and subsequent titration of the reduced solution, cannot fail to have been annoyed by the occasional failure of duplicates to agree. Apparently, in the two analyses everything has been done exactly alike, and yet the results do not agree. Every thoughtful chemist cannot fail to have felt at such times that somewhere in the method there were conditions affecting the result that were not fully controlled. During the last six or eight months in our laboratory we have apparently struck one of these hitherto uncontrolled conditions, whose influence is not large, and yet enough at times to cause annoying discrepancies in duplicates, or between different chemists working on the same sample.

In order to make clear what follows, it should be stated that in the ordinary working of this method the yellow precipitate, after careful washing, is dissolved in ammonia, and this solution is then treated with sulphuric acid largely in excess and diluted to a definite volume, in which condition it is passed through the reductor and subsequently titrated with standard potassium manganate. The reductor in common use consists, as is well known, of a tube of heavy glass, about five-eighths of an inch internal diameter, and about a foot long, filled with powdered zinc, the top being fitted with a funnel, and the bottom with a stopcock. Below the stopcock a smaller tube carries the rubber cork by means of which the reductor is fitted to the flask which receives the reduced solution. This smaller tube usually projects into the flask an inch or two, and it is customary to

use the pump to draw the liquid through the reductor. This much being premised, we may say that in a communication from Mr. Porter W. Shimer, one of the members of the Sub-Committee on Methods of the Committee on International Standards for the Analysis of Iron and Steel, he, among other things, called attention to the fact that when making a number of determinations on the same sample, all other things being the same, he got a reduced solution that required more permanganate if he prolonged the small tube below the stop-cock in the reductor, nearly to the bottom of the flask, than if this small tube projected only an inch or two into the flask. This statement brought afresh to our minds a thought that every one who has worked much with molybdic acid must have had: viz., that reduced molybdic acid is very easily reoxidized. We accordingly determined to find out, if possible, whether this was actually the case, and, if so, how much this difficulty might amount to. Accordingly, a stock solution of ammonia molybdate dissolved in water was prepared, and a number of aliquot parts of this solution measured out. Now, obviously, there are two chances for the reduced solution to become oxidized by exposure to the air. One of these is from the air in the flask during the reduction, and the other from the outside air during the titration. Without going into minute detail, it is, perhaps, sufficient to say that when we reduced an aliquot part of our stock solution, using the short tube of the reductor and adding the permanganate drop by drop, with continual agitation during the whole titration, we used 22.7 cc. of our standard permanganate, all figures given being a mean of a number of closely agreeing determinations. When now we made the reduction the same as before, viz., with the short tube of the reductor, but titrated by allowing about ninety-five per cent. of the permanganate required

to run into the flask before agitation at all and finishing the titration drop by drop, we used 23.1 cc. of permanganate; in other words, so sensitive is a reduced solution of molybdic acid that it is easy by varying the mode of titration to introduce considerable error. Prolonging now the tube at the bottom of the reductor as suggested by Shimer, which would result, as is apparent, in a diminished exposure of the reduced solution to the air in the flask before titration, we found our aliquot part to use up 23.6 cc. of permanganate. But even with the prolonged tube there is some exposure of the reduced solution to the air during the reduction. Accordingly, on the suggestion of my principal assistant, Mr. F. N. Pease, we put a measured amount of standard permanganate solution into the flask which was to receive the reduced solution, more than sufficient to react with it, and then prolonged the tube from the reductor, to dip below the surface of this permanganate. Obviously, with this arrangement the reduced solution is entirely prevented from air exposure. On making the reduction and titrating the excess of permanganate with standard solution of ferrous sulphate, it was found that the aliquot part had now used up 24.1 cc. of permanganate, an extreme difference in amount of permanganate used under the varying conditions described, of nearly six per cent. Obviously, if two chemists were working on the same sample of molybdic acid, one employing the manipulation first described, and the other that last described, the discrepancy between them would be serious. The discrepancy on phosphorus in steel, while the same in percentage, is very much smaller in actual figures, but still enough to be annoying. The work above referred to is not yet quite finished, but enough has already been done to demonstrate that the ordinary method of determining phosphorus in steel can be advan-

tageously modified in the interests of greater accuracy; and also, although not yet rigorously demonstrated, there are strong indications that molybdic acid (MoO_3) is always reduced by zinc to Mo_2O_3 , and that the more complex formulas, $\text{Mo}_{12}\text{O}_{19}$, $\text{Mo}_{24}\text{O}_{37}$, etc., so commonly given as representing this reduction, simply mean that the conditions under which these formulæ were obtained permitted the reoxidation of the reduced solution to the extent indicated.

There is another phase of this question we are discussing: 'The Dignity of Analytical work,' which will, perhaps, bear a few words. It seems to be universally conceded that the brain that plans and guides is worthy of more honor than the hand that executes; the general deserves more than the private soldier; the architect than the builder; the investigator who plans the work than the chemist who makes the analyses. Few will object to such a distribution of rewards as this, and certainly no one will claim that a chemist who, machine-like, simply follows directions, without thought or interest in the matter, can fairly claim recognition for anything more, perhaps, than manipulative skill and honesty. But, on the other hand, it is fair to say that such analysts can truly be called analytical chemists. Does not the genuine analytical chemist embody within himself, not only the capacity of brain to wisely plan his method of attack, to conceive which one of the possible reactions in the case it will be best to employ, but also the requisite manipulative skill, to carry out the line of action decided upon. To my mind, these two things, viz., the brain power necessary to plan the work, together with continual activity of the brain while the work is going on, and the skilled and trained hand requisite to do the work, are necessarily coexistent at the same time in the good analytical chemist, and woe be to that chemist who tries to put them asunder.

The analyst whom chance or the exigencies of earning his livelihood have thrown into a situation where day after day he must, for a time at least, do the same thing over and over again, and who does not, even in this situation, use his brain constantly, does not each time he adds a reagent think what is going on in the beaker; does not each time he washes a precipitate think what he is washing out; does not every time he makes a weight take a genuine interest in the result, and even the hundredth time that he makes the same determination is not on the lookout for some flaw in the method he is using, or some possible new reaction in connection with it—such an analyst, I say, will stand a good chance to remain a routine chemist all his life.

On the other hand, what shall we say of those chemists who plan out a line of investigation and are content not to make the necessary analyses themselves? We are quite well aware that at the present time this is a very common method of making investigations, and we can, of course, understand that pressure of other duties may make it impossible to pursue investigations in any other way. But we cannot regard this state of affairs as, to say the least, anything less than unfortunate. If we may trust our own experience, the time spent in making the analyses required by one line of attack on a stubborn problem is most valuable, in the opportunity which it affords for carrying the problem in mind, and planning out other lines, in case the one in hand does not succeed. Moreover, still more valuable is it to make the analyses yourself, in that while doing so you so frequently get suggestions from the work that are the very ones upon which final success depends. I wish there were time to illustrate this point as its importance deserves, but the history of chemistry and your own experience will have to furnish

them to you. To our minds it is hard to overestimate the importance, especially to a young investigator, of his doing his own analytical work for himself. If we read rightly, this was the almost universal habit of the old masters of our science, and we greatly fear that those chemists who from choice delegate their analytical work to others will find, after years of such delegation, that their reward of successful investigations is very small.

A single thought further. At the present time so much applied chemistry is either based on analytical work, or has analytical work as an almost essential constituent of its existence, that in a paper discussing analytical work a few words may not be amiss on the relations between pure and applied chemistry. Without wishing to touch in the slightest degree on mooted or disputed questions, it may not be unfair to say that, while the applied chemist does truly, as the name indicates, in the mass of his work, utilize or apply the discoveries of others to useful effect, it does not at all follow that in the field of applied chemistry no discoveries yet remain to be made. It is certainly not too much to say that no thoughtful chemist has ever worked for any length of time in any field of applied chemistry without finding himself surrounded with problems involving new and unknown reactions; with problems, am I not safe in saying, requiring for their solution as good appliances, as deep study and as keen thought as any that occupy the minds of the pure chemists. These problems continually force themselves upon him, and his only regret in the matter is that the time at his disposal does not permit him to solve them as fast as they arise. A prominent feature of these problems in applied chemistry is worthy of close attention, viz., they generally have immediate useful applications as soon as they are solved. The applied chemist usually makes an excur-

sion into the unknown, because some difficulty has arisen in the course of his regular work, or because some new, more rapid, or more economical method of accomplishing results is desired. He may succeed in finding a new reaction or in utilizing an old one, as the basis of a successful commercial process, or in modifying a manufacturing method in the interests of both economy and speed. But whatever his work, the immediate useful application of the information he secures is both his stimulus and guide. He may not be able, from lack of time, to follow his work up, and find the complete relations of the facts ascertained to the other branches of chemistry, but this is his misfortune rather than his fault, and this condition of affairs, viz., being unable to follow out to completion lines of research one started on is, if we understand the matter rightly, not characteristic of the applied chemist alone. This much being said, let us ask in what respects the pure chemists resemble or differ from those who work in the field of applied chemistry.

They certainly are alike in this, that neither of them can devote his whole time to original work, but both must devote no small portion of their energy to other lines than making investigations. There may have been a time in the history of chemistry when investigators were so fortunately situated that they could devote their whole time and energy to finding out new truth and giving their results to the world. All honor to such investigators. Moreover, we all know that occasionally an appropriation of funds or an endowment is made for research in some special field. But truly, would it not be too much to say that the work of any large percentage of the pure chemists of to-day is the result of any such fortunate circumstances? Furthermore, the pure and applied chemists are alike in that in their original work both

are seeking for the truth, and if they are successful both are adding to the sum of human knowledge.

They differ, as it seems to me, principally in this: First, the researches of the applied chemists being largely made in the interests of corporations or manufacturing establishments, the results of these investigations in many cases are not at once available to the world, except in so far as they lead to diminish cost of production. Those who have paid for these researches naturally feel that they should be allowed a period of time at least to recoup themselves for their expenditures, and so they protect themselves either by patents or secrecy. But this is only a knowledge of the truth deferred. Sooner or later the results of the investigations of all applied chemists are added to the great body of accumulated chemical knowledge. The pure chemist, on the other hand, at once gives the results of his investigations to the world, and is quite content if the publication of his researches shall bring him as his reward a modicum of appreciation from his fellows. Second, in their original work, the pure chemists differ from the applied chemists in the ulterior purpose for which the investigation is undertaken. As has already been stated, the applied chemist usually undertakes an investigation, tries to find new truth with the avowed purpose of at once utilizing this truth as soon as it is found. Not so the pure chemists. The problems which they attack and solve so successfully have no necessary relation to subsequent utility. The truth which they discover and put on record may be found to be useful at some time, but its possible immediate utility or non-utility is not taken into consideration by the pure chemist, either in his choice of a subject for investigation or in the prosecution of his work. The truth for the truth's own sake is his motto and guiding star.

If we have diagnosed the case correctly, then, the principal differences between the pure and the applied chemist are that the latter withholds the results of his work from the world for a period of time, while the former gives his at once, and that the latter is, in his original work, seeking for truth that is useful as soon as it is worked out, while the former neither knows nor cares whether the truth that he discovers is either now or at any future time turned to practical or useful effect. Let me not be misunderstood. I am not attempting to belittle in any sense the work of the pure chemists. They are worthy of all honor and respect. But, on the other hand, I am not at all willing to have the work of the applied chemists made light of, or treated as though it were in an inferior field. To my mind there is no occasion for either to belittle the work of the other. The field of chemistry is so broad, the amount of unoccupied ground in every branch of the science is so great, that there is neither time nor energy for struggling as to who is greatest or who is least, but in whatever line a man's tastes, opportunities or the force of circumstances may lead him, whether as a pure or an applied chemist, whether organic or inorganic, whether theoretical, physical or agricultural, whether analytical or synthetic, provided in his mind at all times the love of truth is above all, and honest work is being done, he is worthy of recognition, honor and respect.

C. B. DUDLEY.

ALTOONA.

THE AMERICAN MORPHOLOGICAL SOCIETY.

THE eighth annual meeting of the American Morphological Society was held at Cornell University, Ithaca, N. Y., December 28th, 29th and 30th. The following new members were elected: Professor J. H. Comstock, Cornell University; Mr. Ulric Dahlgren, Princeton University; Professor